

refers to the more highly placed relatives of the newly elected King of Norway.

Haakon VII., King of Norway (b. 1872).

$f_{a_{15}}$	Frederick, Crown Prince of Denmark (b. 1843).
$f_{a_{15}} f_a$	Christian IX., King of Denmark.
$f_{a_{15}} bro_2$	George I., King of the Hellenes (b. 1845).
$f_{a_{15}} si_2$	Dagmar, widow of Alexander III., Tsar of Russia, who d. 1894.
$f_{a_{15}} si_2 son_1$	Nicholas II., Tsar of Russia (b. 1868).
$f_{a_{15}} si_1$	Alexandra, Queen of England (b. 1844).
$f_{a_{15}} si_1 son_1$	George, Prince of Wales (b. 1865).
$f_{a_{15}} si_1 da_3$	also <i>wife</i> , Princess Maud (b. 1869) of England.

The formulæ are to be read thus:—"his (the K. of Norway's) father is the 1st (eldest) son, and is Frederick, C.P. of Denmark; "his (the K. of Norway's) father's father is Christian IX."; . . . "his father's 2nd sister's 1st son is Nicholas II."; . . . "his father's 1st sister's 3rd daughter, who is also his (the K. of Norway's) wife, is the Princess Maud." These foot-figures need not interfere with the simplicity of the general effect, while they enable a great deal of additional information to be included.

FRANCIS GALTON.

Atomic Disintegration and the Distribution of the Elements.

MR. DONALD MURRAY's letter (p. 125) deals with a subject which I have been attempting, now for more than a year, to attack experimentally. A similar experience to that which Mr. Murray describes as the experience of a lifetime occurred to me eighteen months ago in a visit to the gold mines of Western Australia. Since then my thoughts have been less concerned with the radio-elements than with those like gold, platinum, thallium, indium, &c., which resemble radium in the minuteness and approximate constancy of the proportion in which they occur in nature.

It is wonderful to reflect that mankind for thousands of years has been passionately and determinedly engaged in the search for gold, not on account mainly of its useful qualities, but on account of its comparative scarcity. The history of gold-getting presents a strange uniformity. The search has been rewarded always with about the same qualified measure of success, never with such success that the value of gold has seriously depreciated. The common saying that about the same amount of gold has to be put into the earth in order to dig it out holds an economic and probably a scientific truth. For may we not consider that the history of these centuries of search, carried on with a tenacity of purpose and a continuity approached in the case of no other element, shows clearly that a natural law is here involved no less than in the case of radium or polonium? The history of gold-getting appears to be substantially the same in all countries in all times. We have the initial prospecting in which the chances and difficulties are so great that only the most adventurous attempt it; the discovery of surface gold and the rush from all parts of the earth; the phenomenal finds and the invariably much greater proportion of failure; the tracing of the gold to its source and the discovery of some cubic acres, or it may be miles, of gold-bearing earth. Then at first only the deposits averaging several ounces to the ton are thought worthy of attention; but these rapidly give out, and attention is directed to the poorer and still poorer veins, while at the same time the steady progress and evolution of the pioneer camp, where often gold seems to be commoner than water, into the civilised community served with railways, electric power, and often elaborate water supply, cheapens the cost of extraction to such an extent that deposits averaging only a few grains to the ton can be made to yield a profit. Finally, we have the same inevitable end when science and organisation have done all in their power, and the remaining ore contains just so much gold as *not* to pay.

Let the case be stated a little differently. What would be the effect of the sudden discovery in any one place of some really large quantity of gold? There seems no doubt that utter chaos would ensue in the commercial world, which might involve before it was got under control a rearrangement of the map of the world. Since nothing of

the sort has ever happened, in spite of the most unprecedented struggles to that end, it is in accord with the principles of natural evolution to conclude that such a contingency probably violates some law of nature. Thus the gentlemen in charge of the national exchequer and of the Bank of England, who on a casual examination appear to be placing the most blind and implicit confidence on the future continuance of the existing order of things, are in reality secure in a fundamental if previously unrecognised law of nature. Eighteen months ago, after my visit to the gold deposits of Western Australia and New Zealand, and by the information which all concerned in the industry so readily placed at my disposal, I became convinced that in all probability gold, like radium, is at once the product of some other parent element, and is itself changing to produce "offspring" elements, so that its quantity, and hence its value, was fixed simply as the ratio of these two rates of change.

My experiments with gold have been both by the direct and indirect methods. The former have been dogged by misfortune and have so far been without result, while in the indirect experiments on ancient gold the results until now have been conflicting. Certainly some nuggets did not contain helium in appreciable quantities, while in others I did find a minute quantity of helium. This, however, was before the elaborate precautions afterwards employed had been adopted, and as I can now repeat the experiments with certainty as soon as occasion permits I am keeping a quite open mind. On the other hand, I have established to my own satisfaction that helium is an invariable constituent of native platinum in all the samples I have tried. The above reasoning, from rarity, after extended search, applies to platinum to a degree only less complete than in the case of gold.

The experiments with the other elements have not yet been proceeding long enough to have furnished results, but I have made a great many experiments with uranium and thorium in the attempt to detect directly the production of helium from these elements. These elements have been, in fact, the standards, for their rate of change is accurately known, and, assuming with Rutherford that the α particle is an atom of helium, may be expected to yield helium at a known rate. The methods of search have been perfected in the case of these two elements, and I am glad to be able to say that it is now only a question of time and patience before the rate at which helium is being produced from these two elements is accurately measured. On the other hand, if helium is not being produced, the experiments will indicate a maximum possible limit of the rate of production (set by the smallest quantity of helium detectable) far below the rate to be expected from theory. This method, which is, of course, applicable to any other element, would detect any other gas of the argon-helium family if produced. So far, however, I have only had one completely successful experiment with each element. In the case of uranium the result was positive, and indicated a rate of production of the same order as that required by theory. In the case of thorium, the experiment was of the nature of a blank test, and it proved that the rate of production is certainly not greater than ten times that required by theory.

Mr. Murray's letter induces me to put on record these imperfect results, and I do this the more readily as they may perhaps serve to emphasise and support his suggestion that experiments along the lines and on the scale he suggests should be carried out. But what laboratory in England could deal with ten tons of lead over a term of ten years?

After a year's work, I confess I am less hopeful than I was of the ability of the individual worker to carry out direct experiments in this subject of atomic disintegration. I wonder if the individual with his humble kilogram and his single lifetime is not starting on an almost forlorn hope, and is unduly and unnecessarily handicapped. Due consideration should be given to the supreme consequences that must follow from successful discoveries in this field. Not only is there to be considered the effect such results must exert on the whole trend of philosophic thought, but certain definite economic problems would be solved. For example, the proof of the disintegration of gold would reduce the doctrine of bimetalism and the theory of

currency to a branch of physical science, while in the mining industry the results would possess a fundamental significance. For the first time in the history of mineralogical chemistry it is possible, thanks to the researches of Boltwood, Strutt, and McCoy, to predict with considerable certainty the percentage of one element (radium) present if the percentage of another (uranium) is known; and one asks to what this discovery may not grow.

It seems to me that the individual and his single lifetime is too small a stake for the prize in view. Such a work should be national, and carried on from century to century if necessary; and what nation has such a right or such a duty as the one in which the subject of atomic disintegration originated? I confess to a feeling of impatience, to the sense of the inadequacy of the single lifetime, in my experiments on such small quantities of gold as I can purchase, when, disintegrating at the same rate, if disintegrating at all, tons of gold are lying useless in the national bank, their secret—possibly one that it much concerns the race to know—guarded from knowledge by every cunning invention that the art of man may devise. I confess to a sense of indignation that I should have to purchase for my experiments coins and other objects of known antiquity when within the walls of the National Museum lie—mere dead relics as they at present are—one of the finest collections in existence, capable of affording evidence perhaps of a longer history than any dreamed of by the antiquarian, and guarded by those who cannot interpret the cypher, and who, officially at least, are unaware of its existence. I confess to a feeling of misgiving in starting experiments where, on the scale possible to the individual, the chances are all against their yielding a positive result in a lifetime. Surely considerations of this character, the availability of the national resources and antiquities for the purpose of scientific investigations under due safeguards, and the provision for and care of experiments of long period with great quantities demanded by this new subject, are worthy of the attention of the nation, and of the British Science Guild as its newly formed adviser.

FREDERICK SODDY.

The University, Glasgow, December 9.

THE suggestion which Mr. Murray has put forward (p. 125) in explanation of the constancy of association of lead and silver has occurred to me also, and is indicated in an article which will probably appear shortly in the "Jahrbuch der Radioaktivität und Elektronik"; some calculations are contained therein which may be of sufficient interest to justify reproduction here.

Some recent experiments¹ have afforded evidence that the activity of the ordinary metals is caused by the emission of α particles. On the assumption that these α particles have an ionising power similar to that of those from radioactive elements, it appears that lead should emit less than one such particle per second. In order to find the maximum rate of change that we can attribute to this metal, we will assume that the emission of one such particle involves the breaking up of one atom of lead and the formation of one atom of silver; thus one atom breaks up per second. Now a gram of lead contains about 4×10^{21} atoms, and therefore to transform one ten-millionth part of the lead would require 4×10^{14} seconds or more than ten million years. Since it would be impossible to detect a smaller proportion than this by chemical tests, I fear that the experiment which Mr. Murray suggests is impracticable. The earth would probably have ceased to be a habitable globe by the time that the lead was ripe for examination; perhaps we may trust posterity to settle the matter with greater expedition!

But the slowness of the change in lead presents serious difficulties to the theory that the silver in galena is a disintegration product. Even so small a proportion as one in ten thousand ($3\frac{1}{2}$ ounces to the ton) would mean that the silver had been accumulating for a thousand million years—a period longer than that usually assigned as the age of the earth. But until we know more of the processes by which deposits of ore were formed, it is impossible to

¹ The accounts of these should be included in an early number of the *Philosophical Magazine*.

say whether the lead could have retained its silver through all the vicissitudes of its career. I believe that the silver cannot be separated from galena by any physical means; it may be so intimately associated that geological processes cannot affect it; but against this we have to set the fact that cerussite often contains much less silver than the galena from which it is obviously derived. But here chemical separation may have taken place involving the passage of the metals into solution.

There are problems connected with the "traces of impurity" constantly associated with certain minerals which await solution by some laborious chemist; it would be interesting to see whether there is any tendency to proportionality like that which holds between uranium and radium. But the absence of such a relation might be explained on the grounds that radio-active equilibrium had not yet been attained.

There is one other point to which attention may be directed. Rutherford has shown that the loss of heat from the earth by conduction would be compensated by the energy evolved by radium distributed throughout the mass of the earth in the ratio of 1 to 2×10^{13} ; it appears that this amount of energy might be supplied by the disintegration of the actual constituents of the earth even if no radium were present. It is becoming clear that the older estimates of the age of the earth, based on physical data, are wholly erroneous; but if the radio-activity of all elements can be established rigidly, and the time constants of their decay measured with sufficient accuracy, it may be possible to use the evidence to which Mr. Murray has directed attention to gain some information as to the period that has elapsed since the solidification of the earth's crust.

NORMAN R. CAMPBELL.

Trinity College, Cambridge, December 10.

IN NATURE, December 7, p. 125, Mr. Donald Murray suggests that the constant association of different elements arises from the slow transmutation of one into the other. The idea is certainly a reasonable one, and I presume has long been in the minds of all who have followed recent work. The writer discussed this question last year (*Chem. News*, 1904, lxxxix., 47, 58, 118), and arrived at Mr. Murray's opinion.

Now interest in the matter is reviving, perhaps I may be allowed to direct attention to this discussion.

Kiel, December 10.

GEOFFREY MARTIN.

Action of Wood on a Photographic Plate.

I HAVE recently seen some photographic plates used at the last eclipse which have on them, not only pictures of the sun, but also pictures of the wood forming the dark-slides in which they had been placed.

At a former eclipse I understand a similar disaster occurred. It may, therefore, be well for me again to state that wood in contact with, or in near proximity to, a photographic plate, even in the dark, can impress upon the plate a clear picture of itself.¹ This action is much stimulated by high temperature and brilliant sunshine. It can, however, be stopped in several ways; probably the simplest one would be to make the slides of copper in place of wood.

WILLIAM J. RUSSELL.

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Magnetic Storms and Auroræ.

THE interesting paper by Dr. Chas. Chree in your issue of November 30 (p. 101) is inaccurate in one particular. He states that the storm of November 12 was not accompanied by auroræ. My friend Mr. John McHarg, of Lisburn, writes me that "it was fairly prominent, to be seen easily above the moonlight, the usual type, a steady glow brighter than the Milky Way, extending half round the horizon and fading off upwards at an altitude of 20° , or 30° in the west."

From that station auroræ were also observed on November 14, 15, 16, 17, 20, 21, 22, 23, 26, 27, and 30, and it is reported also that a bright crimson arch was seen on the early morning of December 1.

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¹ *Phil. Trans.*, vol. cxcvii. p. 281; *Proc. Roy. Soc.*, vol. lxxiv. p. 131.